Internet Voting and Turnout: Evidence from Switzerland

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Abstract

Internet voting (i-voting) is often discussed as a potential remedy against declining turnout rates. This paper presents new evidence on the causal effect of i-voting on turnout, drawing on trials conducted in two Swiss cantons: Geneva and Zurich. Both Geneva and Zurich constitute hard cases for i-voting, given that i-voting was introduced in the presence of postal voting. However, this setting allows us to test some of the more optimistic claims regarding i-voting's ability to increase turnout. Empirically, we exploit the advantageous circumstance that federal legislation created a situation coming close to a natural experiment, with some of Geneva's and Zurich's municipalities participating in i-voting trials and others not. Using difference-in-differences estimation, we find that i-voting did not increase turnout in the cantons of Geneva and Zurich.

Key words: Internet voting; electoral turnout; electoral participation

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1 Introduction

In many advanced democracies, turnout rates have decreased over the past few decades. To counter this trend, many countries are forging ahead with convenience voting reforms, such as Electoral Day registration and postal voting, that aim to simplify the voting process and thus increase political participation (Gronke, Galanes-Rosenbaum, Miller & Toffey 2008). Internet voting (i-voting), a voting method that allows voters to cast their vote remotely over the Internet, is increasingly discussed in this context.[1]

I-voting is less than two decades old, but it already has a turbulent history behind it. Cyber enthusiasm was widespread around the turn of the millennium, and i-voting widely heralded as the 'magic ballot' that would entice many more people to vote (Alvarez, Hall & Trechsel 2009, Gibson 2005, Norris 2005). With great hopes, i-voting was for the first time trialled in the context of a binding election in 2000 at the occasion of the Arizona Democratic Primaries. Soon experimentation started in other countries as well. In 2002, the first i-voting experiments were conducted in the UK, and in 2003 i-voting experimentation began in Canada and in Switzerland. Estonia followed in 2005.

Against the initial enthusiasm, most of today's polities continue to vote offline. Several countries have abandoned experimentation with i-voting after the first few trials. Examples include the US, the UK, the Netherlands, Austria, and Norway. The reasons are diverse and include concerns related to the security of voting online, but also more prosaic worries that i-voting might help political opponents (Hall 2015, Mendez & Trechsel 2005, Mendez 2010). Furthermore, initial, often wildly exaggerated hopes of immense turnout increases have clearly not materialized (Bochsler 2010, Goodman 2014).

Nevertheless, i-voting continues to be practiced in several corners of this world. Estonia has fully generalized i-voting and uses it in all national and local elections. In several other countries, experimentation with i-voting continues at a more limited scale, typically in local or regional settings. Examples include Canada, Australia, France, Brazil,

[1]The term 'e-voting' is also often used. We prefer i-voting because there are many forms of electronic voting (e.g. kiosk voting, punch card, and optical scans), not all of which involve casting one's vote remotely over the Internet (Gibson 2005, Pammett & Goodman 2013).

and Switzerland. Further, many citizens continue to view online elections favorably, and arguments in favor of i-voting continue to be frequently made by both journalists and politicians (see e.g. Castella 2015, Goodman 2014, Hall 2015, Milic, McArdle & Serdült 2016). There are several reasons. In a world where people shop, date, and bank online, paper-and-pencil elections appear somewhat archaic. Online elections promise increased speed, efficiency, and accuracy in terms of the vote counting, but also increased accessibility for the disabled and elderly. And, perhaps most importantly, many continue to argue that i-voting increases turnout, if maybe not to the phenomenal rates suggested by some in the early days of i-voting (Pammett & Goodman 2013).

Given that discussions about i-voting are likely to stay with us in the foreseeable future, academics should provide policymakers and the general public with evidence regarding the pros and cons of i-voting. Clearly, i-voting raises security issues, making it important that computer scientists investigate the risks associated with voting online and how they can be tackled (see e.g. Simons & Jones 2012). The main contribution political scientists can deliver is evidence regarding the turnout effect of i-voting. How large a turnout increase can we expect if i-voting is introduced? Or can we even expect any?

Existing evidence points in different directions. Some have found large turnout increases, others more moderate ones. Still others found no turnout effect at all, thus questioning one of the key justifications for i-voting. However, the lessons that can be drawn from existing studies are limited due to methodological problems. In this study, we present new evidence on the effect of i-voting on turnout, drawing on i-voting experiments conducted in two Swiss cantons: Geneva (2003–ongoing) and Zurich (2005–2011). These two cases offer an advantageous setting to learn about the causal effect of i-voting on turnout. That said, they notably constitute difficult terrain for i-voting. In both Geneva and Zurich, i-voting was introduced in the presence of postal voting, which decreases chances for an i-voting turnout effect. Still, this setting allows us to test some of the more optimistic claims regarding i-voting's ability to increase turnout.

Empirically, we exploit federal legislation that limited the number of citizens allowed to take part in i-voting trials and thus created a situation resembling a natural experiment,

with some of Geneva's and Zurich's municipalities taking part in trials and others not. The resulting within-canton variation in the availability of i-voting allows us to circumvent many of the challenges to causal inference prevalent in previous studies. Drawing on difference-in-differences (DID) estimation, we find that the introduction of i-voting did not affect turnout in the Swiss cantons of Geneva and Zurich. Implications for policymaking as well as future research are discussed in the conclusion.

2 Existing Evidence

The effect of i-voting on electoral turnout remains an open question. Several existing studies have examined the nexus between i-voting and turnout, but the conclusions vary dramatically from study to study. Some have come to highly optimistic conclusions. For example, Spada, Mellon, Peixoto & Sjoberg (2015) analyze an i-voting experiment in the context of a budget referendum in Brazil and find that i-voting increased turnout by more than 8 percentage points. Solop (2001) comes to a similarly optimistic conclusion regarding the previously mentioned i-voting experiment in the context of the 2000 Arizona Democratic Primaries. Other studies report much smaller but still significant effects. Trechsel & Vassil (2010), for example, estimate that the availability of i-voting increased turnout by 2.6 percentage points in Estonia's 2009 local elections. Still others found no turnout effect at all. Bochsler (2010), for example, concludes that i-voting did not raise turnout in Estonia's 2007 national elections, whereas Segaard, Baldersheim & Saglie (2013) conclude that i-voting did not affect electoral participation in Norwegian local elections.

A possible explanation for the disparate findings is that i-voting's effect on turnout depends on contextual factors, such as election salience (Karp & Banducci 2000). However, the lessons that can be drawn from existing studies are limited because most suffer from at least one of three methodological problems. First, several existing studies draw inferences based on survey items asking voters whether the availability of i-voting increased their chances of participating (e.g. Trechsel & Vassil 2010, Spada et al. 2015, Gerlach &

Gasser 2009, Nemeslaki, Aranyossy & Sasvári 2016). However, subjective impact evaluations are problematic because voters may not be the best judges of the causes of their own behavior.

Second, many existing studies focus on pilot projects (e.g. Solop 2001, Spada et al. 2015, Segaard, Baldersheim & Saglie 2013). The reason is obvious: the number of cases that have a prolonged experience with i-voting is small. That said, there are good reasons to believe that the findings from pilot projects may not replicate in the longer run. I-voting requires voters to engage with new security measures, and voters may be unwilling to pay the cost to learn the new system for a one-shot event (Hall 2015). The bias in pilot projects may also run the other way. I-voting pilots tend to catch unusual media attention and are often actively promoted by the authorities who try to alert voters to the new mode of voting, which may increase voters' willingness to get involved (Gibson 2005).

Finally, turnout is driven by many factors other than i-voting. Thus, it is tricky to determine whether changes in turnout should be attributed to the shift to i-voting or to other factors. Consider Norris's (2005) evaluation of an innovative experiment conducted in the context of the 2003 English local elections in which 59 different English local districts tested alternative ways of facilitating the voting process, including i-voting and postal voting. Interestingly, Norris finds that while mail ballots were highly effective at boosting turnout, online ballots proved less successful, thus casting doubt on claims that i-voting constitutes the ultimate in convenience voting (see below). However, Norris estimates turnout effects based on simple comparisons of turnout rates before and after the 2003 experiment. Thus we cannot be sure whether Norris' findings are real or due to unobserved election-specific dynamics, such as the closeness of the race. Analogous problems emerge, for example, in Goodman's (2014) study of i-voting in the context of Canadian local elections and (Segaard, Baldersheim & Saglie 2013) study of local elections in Norway. Crucially, accounting for factors such as the closeness of the race remains difficult even with more complex statistical techniques since concepts such as the closeness of the race are notoriously difficult to measure (Keele & Minozzi 2013).

We contribute to the literature with a new analysis of the turnout effect of i-voting

trials in i-voting trials in two Swiss cantons, Geneva and Zurich. Both cases under study constitute difficult terrain for i-voting. This is because fully generalized, automatic postal voting, whereby voters are automatically mailed their ballot papers and can return them by post, was introduced in both cantons in the mid-1990s before i-voting experimentation started in Geneva (2003) and Zurich (2005), respectively (Luechinger, Rosinger & Stutzer 2007). We cannot thus draw inferences regarding i-voting's turnout effect in a more "standard" setting where otherwise only the traditional voting at the polling station is possible. Still, we can test some of the more optimistic expectations regarding i-voting's ability to increase turnout (see below). Furthermore, the trials in the cantons of Geneva and Zurich offer an unusually advantageous setting to draw robust causal inferences. In both Geneva and Zurich i-voting was made available on a regular and extended basis, thus circumventing the ubiquitous pilot project problem. Further, in both Geneva and Zurich federal legislation limiting the share of a canton's voters taking part in i-voting trials created a natural experiment-like setting, with some municipalities participating in trials involving federal referendums and others not. The resulting variation allows us to make within-canton comparisons of actual turnout rates across municipalities that all vote on the same issues (federal referendums). This has important advantages in terms of causal identification since by design, most potential confounders can be held constant. By observing the same administrative units over time (municipalities), we can eliminate potential confounding due to unobserved local-level variables, such as local political culture. On the other hand, by observing the same units voting on the same issues, unobserved election-specific factors can be held constant. This set-up allows us to estimate turnout effects that are not vulnerable to many of the threats to causal inference in previous studies.

Existing work on i-voting's effect on turnout in Geneva and Zurich has come to varied conclusions, but none has fully exploited the favorable setting in terms of causal inference. An early study by Serdült & Trechsel (2006) found that i-voting increased turnout in the canton of Zurich. However, Serdült & Trechsel's findings base on subjective impact

evaluations while also falling prey to the pilot problem.[2] A recent, more sophisticated study of the Genevan i-voting trials comes to contrary conclusions (Sciarini, Cappelletti, Goldberg, Nai & Tawfik 2013). Focusing on actual turnout rates over a long time span, Sciarini et al. find that the introduction of i-voting had no effect on turnout in the canton of Geneva, controlling for district population, age structure, female share, and the type of vote. However, some doubts remain since Sciarini et al. did not make full use of the potential to account for unobserved confounders. Our research design innovates by removing threats to causal inference emerging from all unit- and election-specific confounders, as well as smoothly changing dynamics.[3]

3 Theory

As argued above, in both cases under study i-voting was introduced in the presence of postal voting. Can i-voting increase turnout even in the presence of postal voting? Optimistic proponents of i-voting would suggest so. Indeed, increasing turnout constituted an important motive for i-voting experimentation in the two cantons we analyze (e.g. Beroggi 2014). The primary argument of the i-voting optimists is that i-voting constitutes "the ultimate in convenience voting" (Alvarez, Hall & Trechsel 2009, p. 497, also see Powell et al. 2012). Standard rational choice-inspired theories of electoral turnout suggest that the probability that an individual turns out is partly a function of the costs implied in voting (e.g. Riker & Ordeshook 1968). And there are several reasons why i-voting can be seen as more convenient than postal voting. While postal voting is rather convenient compared to the traditional voting at the polling station, voters still have to leave their homes and find the nearest mailbox. By contrast, with i-voting, anybody with

[2]Serdült & Trechsel (2006) focus exclusively on Zurich's first ever i-voting trial involving a federal vote.

[3]Sciarini et al. introduce crossed random effects for districts and referendum days, but this invokes the strong assumption that the availability of i-voting is uncorrelated with unobserved unit- and time-specific factors (Wooldridge 2010, pp. 289–290). In contrast, we use fixed effects for both municipalities and referendum days, which effectively removes confounding due to unit- and time-specific variables.

an Internet-enabled device can vote from wherever (s)he wants. This may be the comfort of the home, the workplace, or indeed any other place, given the spread of Internet-enabled mobile devices. This is likely to constitute an advantage especially for those with limited mobility. However, it may also increase turnout more generally. For example, existing evidence suggests that bad weather conditions can decrease voter participation (Gomez, Hansford & Krause 2007). I-voting makes voting more convenient if there is, say, rain or snow. An additional advantage of i-voting is that it implies an extended voting deadline. With postal voting, voters have to send off their voting materials several days before Election Day to make sure that their votes are counted. Internet votes, contrary to snail's pace postal votes, are delivered immediately and can thus be submitted closer to Election Day.[4] Finally, depending on whether postal ballots require postage stamps, i-voting may save voters the expenditure and effort implied in organizing a postage stamp.[5] In sum, optimists would expect that i-voting increases turnout even in the presence of postal voting because it further reduces the transaction costs implied in voting.[6]

A second argument offered by some proponents is that i-voting increases the attractiveness of voting. According to this line of thinking, traditional paper-and-pencil forms of voting are increasingly seen as out-of-date, especially among the wired younger generation. Moving elections online is expected to raise the appeal of voting, and create a turnout effect due to the "pull" of the Internet (Gibson 2005, Vassil & Weber 2011). Again, if true this would suggest that i-voting increases turnout even in the presence of postal voting.

However, there are good reasons to be skeptical about i-voting's superior ability to

[4]In Switzerland, online votes must for safety reasons be submitted by noon on the day before election or referendum votes. The logic is that this would allow online voters enough time to cast another vote at the polling station if evidence emerged that the system was hacked.

[5]In Switzerland, some municipalities provide pre-stamped postal ballots, while others do not.

[6]Depending on the implementation, i-voting may also decrease information costs, for example by providing relevant information (e.g. the names of candidates) or by providing an automatic reminder of upcoming elections because voters are sent security keys before the election. Postal voting offers similar benefits.

raise turnout. Both i-voting and postal voting allow voters to cast their vote remotely, and thus offer similar convenience. I-voting may offer some additional advantages, but for many voters the additional convenience offered by i-voting is small and it may well not be big enough to entice additional voters to the polls (Norris 2005, Sciarini et al. 2013). Initial evidence in this direction may come from the fact noted below that the majority of (residential) voters in both Geneva and Zurich continued to vote by post even if they had the opportunity to vote online.

Furthermore, the assumption that voters flock to the polls due to the appeal of new technology involves a rather high dose of 'blue sky' cyber optimism, especially in terms of its mid- and long-term expectations. It may be that curiosity plays a role in the initial phase when i-voting is introduced, not least given the high levels of media attention that tend to accompany pilot trials. But such effects appear unlikely to persist.

Finally, proponents of i-voting often overlook the implications of security concerns. The transmission of ballots via the Internet raises fears of third party manipulation (e.g. Simons & Jones 2012, Springall, Finkenauer, Durumeric, Kitcat, Hursti, MacAlpine & Halderman 2014). To what extent such fears are objectively justified is an open question (Pammett & Goodman 2013). However, maintaining public confidence in the legitimacy of elections is absolutely crucial. One of many reasons is that a lack of confidence in the integrity of elections is likely to decrease turnout. If voters come to the conclusion that their vote will not make a difference, they have few incentives to participate (Birch 2010, Gerber, Huber, Doherty, Dowling & Hill 2013, Norris 2014). In an extreme case, concerns about the integrity of elections caused by i-voting could therefore even decrease turnout. Luckily, this danger appears to be theoretical, at least in the cases we analyze. Existing survey evidence suggests that while many Swiss citizens have concerns related to the security of online ballots, there are no signs that they have lost confidence in the integrity of elections altogether (Milic, McArdle & Serdült 2016). That said, security concerns also raise a lesser danger. Given that concerns about the security of online elections are widespread, i-voting may not reap its full potential in terms of increasing turnout. Trechsel & Vassil (2010), for example, show that citizens who distrust online

ballots are unlikely to cast their votes online. Thus, some citizens may shy away from i-voting even if they see it as more convenient.

In sum, then, i-voting's ability to raise turnout in the cases we analyze, where i-voting was introduced in the presence of postal voting, is uncertain. Before we turn to the research design and the empirical evidence, the next section gives a short overview of the Swiss Internet voting roll-out.

4 Internet Voting in Switzerland

Switzerland's approach to i-voting is best described as cautious and strongly shaped by its decentralized structures (Gerlach & Gasser 2009, Mendez 2010). In the initial phase, i-voting experimentation was limited to three cantons: Geneva, Neuchâtel, and Zurich. In the spirit of a decentralized approach, each of the three pilot cantons developed its own i-voting system. However, in effect Geneva's and Zurich's solutions are highly similar: Voters access the i-voting web page, enter the personal identification codes that they have been previously mailed, and vote. In contrast, Neuchâtel's i-voting solution is embedded in a larger e-government portal that allows for a broader range of state-citizen interactions, including the filing of tax reports and other administrative tasks. Importantly, to get access to this portal and hence the possibility to i-vote voters must physically identify themselves at the local administration. This additional burden makes i-voting less convenient, rendering a turnout effect less likely. No such registration process is required in the cases we are focusing on. Contrary to Neuchâtel, all voters are automatically mailed their access codes before each election or referendum.

After a number of preliminary tests involving local-level referendums in 2003/2004, i-voting was premiered in a nation-wide referendum in September 2004 in the canton of Geneva. The cantons of Neuchâtel and Zurich joined the trials in 2005.[7] Since then i-voting has been available in approximately 60 separate electoral contests across Switzer-

[7]Note that in an initial test phase (until 2007), Zurich also offered the option to cast a vote via SMS. Roughly 5% of votes were cast via SMS.

land. Starting in 2008, i-voting has also been made available to expatriates, first to those registered in the three pilot cantons and then also to expatriates from some other cantons.

Nevertheless, the Swiss i-voting roll-out has remained piecemeal. First, the three pilot cantons remained the only places where residents were included in the i-voting trials. All other participating cantons have offered i-voting exclusively to expatriates. Note that we cannot consider expatriate trials in our analysis due to data limitations—turnout data for expatriates is in most cases unavailable (Germann & Serdült 2014).

Second, almost all experiments involved referendums. Due to the complexities associated with open list PR elections, which are common in Switzerland, elections were mostly left out, hence also our empirical focus on *referendum votes*.

Finally, as noted above, for national-level trials involving residents the share of voters eligible to vote online has been limited to a maximum of 20% of those residing in a canton (30% since 2012). This limitation aims to control the risk associated with i-voting trials (manipulation etc.) (Pammett & Goodman 2013, p. 21). Notably, the federal limitation did not have larger implications for Neuchâtel. The required extra step of signing up for i-voting allows the authorities to cap voters' access to i-voting if necessary. Thus voters from across the canton were allowed to participate in the trials. The resulting lack of between-municipality variation in i-voting availability renders the case of Neuchâtel less suited to our purposes. In contrast, the cantons of Geneva and Zurich, where voters are automatically sent their online ballots, both had to take action. Specifically, they both chose to limit the number of municipalities participating in the trials. This led to the natural experiment-like setting that we exploit. According to the administrators, trial municipalities were selected in a way that maximizes balance across regions and on a number of socio-demographics, including wealth, urban/rural, and political leanings.[8] This can be expected to facilitate the study of turnout effects. Importantly, the federal cap applies only to federal votes. Thus both Geneva and Zurich have been free to make i-voting available on a broader basis when there is no simultaneous federal-level vote. The

[8]Beroggi (2014), personal communication with Michel Chevallier (former vice director of Geneva's i-voting program).

canton of Geneva (though not the canton of Zurich) has done so on a small number of occasions, offering i-voting across the canton in all 45 municipalities.[9] Hence our exclusive focus on *federal* referendums.

While Switzerland's i-voting roll-out has remained piecemeal, Swiss cautiousness has not proven a guarantee for a smooth implementation process (Mendez & Serdült 2014). Security concerns, legal issues, and fears of electoral losses by conservatives caused Geneva to temporarily suspend its program between 2005 and 2008. Zurich, on the other hand, stopped its program in 2011 due to technical problems. I-voting experimentation resumed in 2015 (this time only for expatriates), though only months thereafter i-voting trials in several cantons, including Zurich, were discontinued as the software did not pass an external security audit. At the time of writing it remained unclear whether or when the trials will continue in these cantons. However, i-voting trials proceed in the cantons of Geneva and Neuchâtel (for both residents and expatriates) as well as Basel-City, Berne, and Lucerne (only for expatriates).

Finally, a short note on the popularity of the online voting channel. Among Swiss residents, the rate of votes cast online tended to hover around 15% and 20% in Geneva and Zurich, respectively, after initially higher rates around 30% probably due to a novelty effect. Given the added burden of registration, the adoption rate has been somewhat lower in Neuchâtel (approximately 10% in recent years, up from 3% in the initial years). In all three cantons postal voting always remained possible and, interestingly, was clearly more popular than i-voting. In Geneva, for example, approximately 80% of votes cast were postal votes. In Zurich and Neuchâtel, the respective figures are 70% and 85%. Meanwhile, only 5–10% of all votes were cast at polling stations. As argued above, the continued popularity of postal voting suggests that few residential voters see big differences between i-voting and postal voting in terms of convenience. That said, i-voting proved significantly more popular among expatriates, with adoption rates generally in the

[9] An important reason for the small number of times Geneva offered i-voting on a broader basis is that separately scheduled lower level votes constitute a relatively rare scenario because regional and local authorities want to profit from federal votes' generally higher mobilization potential.

range of 50% and 70%, depending on the canton (Serdült, Germann, Mendez, Portenier & Wellig 2015).

5 Data and Methods

5.1 Research Design

This study aims to establish the causal effect of the introduction of Internet voting in the Swiss cantons of Geneva and Zurich on voter turnout in federal referendums. Ideally, we would measure the difference in turnout rates under i-voting and no i-voting for the same units and the same referendums. Since this is not possible, we need to impute a credible counterfactual. As noted above, i-voting was constrained to selected municipalities in both Geneva and Zurich. This allows us to exploit variation in the treatment assignment (i.e. i-voting) and the outcome (i.e. turnout in federal referendums) across both municipalities and time and estimate the difference-in-differences (DID) (Ashenfelter 1978, Card & Krueger 1994). Our design is robust to unobserved confounders that are fixed over time, time-specific, or smoothly changing.

DID estimation implies that we compare the difference in the outcomes for treated units before and after the treatment to the same difference for control units. In other words, the change in the outcomes of untreated units before and after the intervention is used as a counterfactual for the respective change for the treated units. A fundamental upshot of DID estimation is that by design DID takes care of unit-level confounders that are fixed over time. In principle, many of the possible confounders, including local political culture, socio-economic strata and rural/urban status, vary minimally over reasonably short time spans. Moreover, DID automatically accounts for time-specific confounders (common shocks). This is an attractive property too: turnout in federal referendums varies widely depending on the issue(s) at hand. DID removes any possible bias due to differential levels of mobilization across referendum days, as long as treated and untreated municipalities are equally affected.

Like any other observational design, DID relies on a strong identification assumption:

In the absence of treatment, differences between treated and control units should have remained constant over time (Keele & Minozzi 2013). This is commonly referred to as the parallel trends (or parallel paths) assumption. While we provide evidence that this assumption is plausible in the two cases at hand below, we weaken it by accounting for municipality-level quadratic time trends. This in addition controls for unobserved smoothly changing confounders, such as slow changes in the socio-economic composition of a municipality. In alternative models, we also directly account for a number of possible confounders. The results remain the same.

We estimate separate models for Geneva and Zurich and do not consider municipalities from any other canton. We do this for two reasons. First, to minimize potential confounding. Cantonal authorities often schedule regional votes simultaneously with federal referendums. This can imply problems if cantonal votes lead to higher levels of mobilization. By looking at each canton separately, we ensure that the effect estimate is not biased due to differential mobilization levels across cantons as a result of cantonal votes.[10] Second, while the i-voting solutions in Geneva and Zurich are similar, there may be small differences regarding implementation and/or differential trust in the security of the systems. Considering each canton separately improves homogeneity of treatment.

Formally, we estimate the effect of i-voting on voter turnout using two-way fixed effects linear regression models including quadratic municipality-level time trends:

$$Y_{it} = \alpha + \beta X_{it} + \gamma Z_{it} + \mu_i + \lambda_t + \epsilon_{it} \tag{1}$$

whereby Y_{it} represents turnout in municipality i at time t, α the constant, X_{it} a dummy that is set to one if i-voting is available and zero otherwise, Z_{it} a vector of municipality-level quadratic time trends that controls for smoothly changing confounders, μ_i a municipality fixed effect that controls for time-invariant unobserved confounders, λ_t a referendum day fixed effect that controls for common shocks, and ϵ_{it} an idiosyncratic

[10]Note that municipal authorities sometimes also schedule municipal-level votes simultaneously with federal referendums. While it is difficult to account for this, municipal votes are rarely drivers of mobilization, and thus unlikely to bias our results.

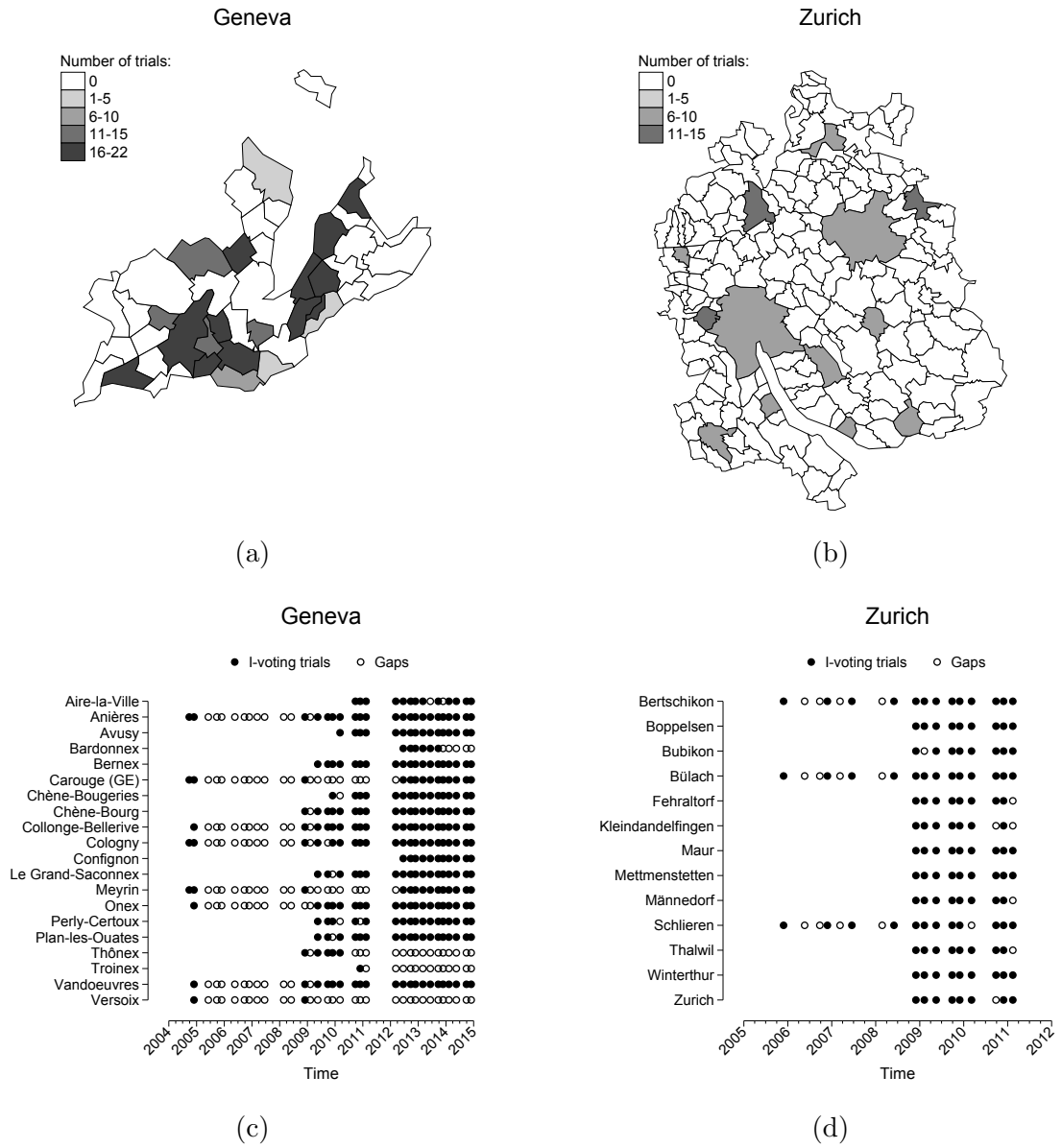
error term. β captures the average effect of the introduction of i-voting, and is thus the parameter of main interest. Since we observe the same units (municipalities) over time, we report standard errors clustered at the municipality level to avoid problems caused by possible serial correlation (Bertrand, Duflo & Mullainathan 2004). In alternative models, we in addition account for possible contemporaneous dependence caused by municipalities voting on the same issues on the same day by estimating standard errors clustered at both the municipality and the referendum day level (Cameron, Gelbach & Miller 2011).

5.2 Data

DID estimation requires information on the outcome of interest both before and after the intervention. Thus, we collected data on turnout in federal referendums starting in 2001, well before the first federal-level i-voting trials in 2004 (Geneva) and 2005 (Zurich), respectively. Turnout is recorded by municipality and in percentages. The unit of analysis is the municipality-referendum day. If voters were asked to vote on more than one issue on the same referendum day, the turnout figure is based on the referendum with the highest participation. For Zurich, the last year we cover is 2011 since this is when the i-voting program was stopped. Geneva, on the other hand, is covered until the end of 2014. In total, our data set contains information on 44 referendum days in 45 municipalities in Geneva and 32 referendum days in 171 municipalities in Zurich. The average turnout figures are 54.79% for Geneva and 48.79% for Zurich.

For all municipality-referendum days, we record whether Internet voting was available or not. Figure 1 gives a basic overview. In Geneva, a total of 20 municipalities (out of 45) participated in the federal trials, with some participating in more than 20 trials as of the end of 2014. Overall, there are 295 municipality-referendum days with i-voting. Note that in some of the trial municipalities, i-voting was not constantly available. As noted above, Geneva's i-voting program was suspended from 2005–2008. Furthermore, in 2008/2009, there was a rotation to accommodate over-demand among municipalities for inclusion in the trials while keeping within the narrow margins set by the federal legislation limiting the number of citizens participating in i-voting trials. There are also some smaller gaps,

Figure 1: Overview of i-voting trials in federal referendums



typically as a result of simultaneous elections that the i-voting system was unable to accommodate. In Zurich, which stopped its program in 2011, 13 municipalities took part (out of 171), with some participating in more than ten trials. In total, there are 121 municipality-referendum days with i-voting. Again, there are some gaps, especially in the initial, more limited testing phase (2005–2008). For more details on measurement, including information on the control variables used in the robustness section, refer to section A in the online supplement.

Table 1: The turnout effect of i-voting

	(1) Geneva	(2) Zurich
I-voting	−0.041	−0.613
	(0.294)	(0.565)
Municipality FEs	✓	✓
Referendum day FEs	✓	✓
Quadratic municipality time trends	✓	✓
Municipalities	45	171
Years	2001–2014	2001–2011
Observations	1980	5472

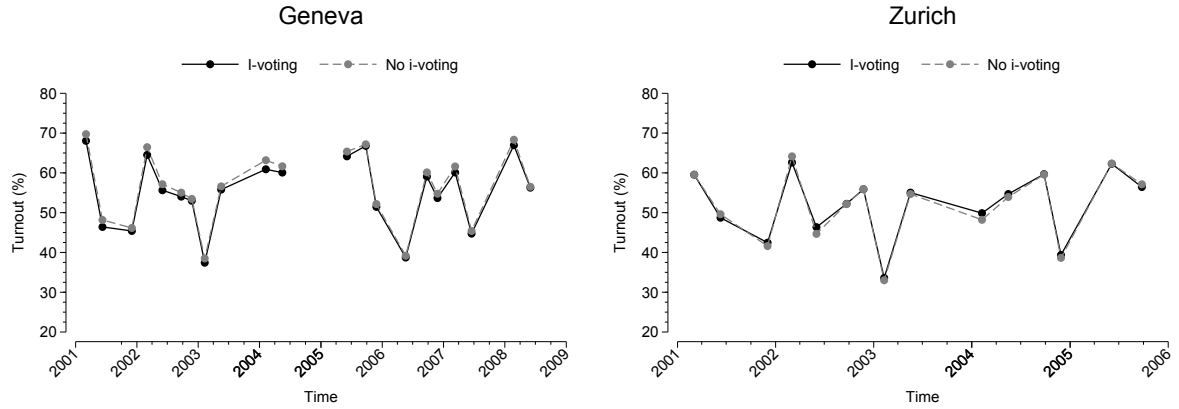
Note: This table shows the results from two-way fixed effects regressions including municipality-level quadratic time trends. The outcome is turnout in federal referendums. FEs stands for fixed effects. Standard errors clustered at the municipality level in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

6 Results

6.1 Main Results

Table 1 presents our main results. The coefficients are based on linear regressions of turnout in federal referendums on the i-voting identifier with municipality and referendum day fixed effects and quadratic municipality time trends. Standard errors are clustered at the municipality level (Bertrand, Duflo & Mullainathan 2004). Column 1 shows the results for the canton of Geneva. We find that the introduction of Internet voting did not increase voter turnout in the presence of postal voting. The coefficient, while negative, is very close to zero and clearly statistically insignificant (p=0.889). A similar picture emerges for Zurich (see column 2). The point estimate is negative, but with a p-value of 0.279 the effect is again clearly statistically indistinguishable from zero. We report tests of the identification assumption and a number of robustness checks further below. Additional robustness checks are reported in the online supplement.

Figure 2: Pre-intervention trends



(a) (b)

6.2 Evaluating the Parallel Trends Assumption

The critical identification assumption in DID designs is that the treatment and control groups (i.e. those municipalities with and those without i-voting) would have had parallel trajectories in the absence of treatment (i.e. had i-voting not been introduced). The parallel trends assumption is not directly testable since it relates to an unobservable counterfactual—we cannot observe turnout under no i-voting after the introduction of i-voting. However, we can evaluate whether treatment and control groups followed parallel trends in the pre-intervention phase. If not a formal test, this can make the parallel trends assumption more (or less) plausible (Angrist & Pischke 2009, Bechtel, Hangartner & Schmid 2015).

We proceed with a simple graphical evaluation. Figure 2 plots the turnout trends in treated and control groups before the introduction of i-voting. All municipalities with at least one i-voting trial were assigned to the treatment group, all others to the control group. For Zurich, we consider the period from 2001 until late 2005, when the first trials started. For Geneva, we consider i) the period from 2001 until mid-2004 before the first phase of the trials, and ii) the period in-between the first and the second trial phase from mid-2005 to mid-2008. Based on Figure 2, the identification assumption appears credible: pre-intervention trends appear reasonably parallel. Indeed, turnout rates are very similar between treated and control groups. In section B of the online supplement we report the

results of a more formal test of the equivalence of pre-intervention trends using placebo treatments. The conclusion remains the same.

6.3 Robustness Checks

6.3.1 Omitted Variable Bias

We now conduct a series of robustness tests to draw additional support to our findings. First, we consider the possibility of omitted variable bias. Even though we provided evidence supporting the parallel trends assumption above, a possible concern with DID remains that there may be time-varying confounders left unaccounted for by the quadratic municipality trends. For instance, bias could emerge due to rapid changes in municipalities' socio-economic composition. We employ two strategies to address possible bias due to omitted time-varying variables. First, we add a number of municipality-level covariates to the specification: per capita income (logged), unemployment rate, the share of voters aged 35 and under, the share of voters aged 65 and above, population (logged), the share of foreigners residing in the municipality, and left-wing parties' vote share in the last national election (see section A in the online supplement for more details).[11] Reassuringly, the results remain the same (see columns 1 and 2 of Table 2).

Second, we re-estimate the main models using matched municipalities. The basic idea here is to reduce heterogeneity by reducing the sample size, that is, considering only control municipalities that are similar to treated units and at the same time excluding treated municipalities that have no comparable control unit. The resulting gain in unit homogeneity reduces the threat emerging from unobserved confounders (Blundell & Dias 2000). In particular, estimation based on matched municipalities reduces potential threats resulting from the possibility that certain referendum proposals mobilize more in some municipalities than in others (e.g. more in richer than in poorer municipalities) and potential bias if municipalities (self-)selected into i-voting experiments due to negative

[11] Unlike below, we do not account for population density because it is collinear with the municipality fixed effects. We also do not account for past turnout levels and past turnout trends to avoid post-treatment bias.

Table 2: Robustness tests

	(1) Geneva	(2) Zurich	(3) Geneva	(4) Zurich	(5) Geneva	(6) Zurich	(7) Geneva	(8) Geneva
I-voting	0.099	−0.629	0.252	−0.511	−0.041	−0.613	−0.383	−0.105
	(0.359)	(0.552)	(0.241)	(0.554)	(0.27)	(0.52)	(0.396)	(0.323)
Municipality FEs	✓	✓	✓	✓	✓	✓	✓	✓
Referendum day FEs	✓	✓	✓	✓	✓	✓	✓	✓
Quadratic municipality time trends	✓	✓	✓	✓	✓	✓	✓	✓
Controls	✓	✓	✗	✗	✗	✗	✗	✗
Pre-processing (matching)	✗	✗	✓	✓	✗	✗	✗	✗
Two-way clustered standard errors	✗	✗	✗	✗	✓	✓	✗	✗
Municipalities	45	171	24	21	45	171	45	45
Years	2001–2011	2001–2011	2001–2014	2001–2011	2001–2014	2001–2011	2001–2014	2001–2014
Observations	1440	5463	1232	704	1980	5472	1935	1935

Note: This table shows the results from two-way fixed effects regressions including municipality-level quadratic time trends. The outcomes are turnout in federal referendums (models 1–6) and turnout among those aged 35 and under/65 and above in federal referendums (models 7 and 8). FEs stands for fixed effects. Standard errors clustered at the municipality level (models 1–4, 7–8) and at both municipality and referendum day level (models 5–6) are given in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

turnout trends (though this appears unlikely based on descriptions from project administrators (see above) and based on our own evidence (see Figure 2). To implement this idea, we pair treated and control municipalities using propensity score matching based on the following municipality-level variables: past turnout level, past turnout trend, per capita income, population size, share of voters aged 65 and over, and population density. We match treated and control municipalities 1:1 with replacement, imposing common support. We then re-estimate the main models on the matched samples. For more details on the procedure refer to section C in the online supplement. Adding further confidence to our results, the substantive conclusion remains unchanged (see columns 3 and 4 of Table 2).

6.3.2 Variance Estimate

Another possible concern is that the variance estimate in the two main models does not account for contemporaneous dependence (Cameron, Gelbach & Miller 2011). In columns 5 and 6 of Table 2 we re-estimate the two main models using standard errors clustered at both the municipality and referendum day level, thus accounting for both serial and contemporaneous dependence. The variance estimates are nearly identical to the main models reported above.

6.3.3 Heterogeneous Treatment Effects

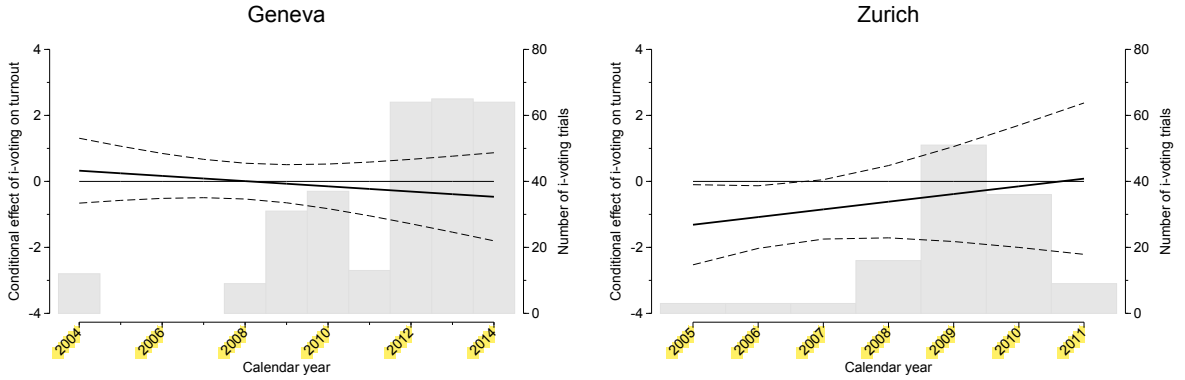
Next we consider the possibility that i-voting disproportionately affects young or elderly voters. I-voting may disproportionately raise turnout among the young because they are especially attracted by new technology. Conversely, i-voting may disproportionately raise turnout among elderly voters because older voters are more affected by mobility limitations. While our main models should in principle pick up a turnout effect even if it is mainly young or old voters that are enticed to vote, it is possible that using turnout among all voters as the dependent variable masks an effect on digital natives/the elderly due to the high numbers of older/younger voters in the sample. In columns 7 and 8 of Table 2 we re-estimate the Geneva model using turnout among voters aged

35 and under/voters aged 65 and over as the dependent variables. Unfortunately, such disaggregated data is not available for Zurich. We find no evidence of a turnout effect on young or old voters.

Finally, we consider the possibility of heterogeneous treatment effects due to a novelty effect or due to variations in the perceived security of i-voting. In theory, the i-voting turnout effect may have vanished over time due to novelty. It is the very first trials that tended to attract the highest numbers of online voters (see above). Once interest in the new technology has ebbed, so may the mobilization effect. Second, the security aspect has become more salient in political debates in recent years in Switzerland, and higher levels of distrust towards i-voting may offset potential turnout gains. Since both mechanisms suggest that i-voting's turnout effect decreases with time, a straightforward way to test for them is to interact the treatment indicator with the calendar year.[12] Following Brambor, Clark & Golder (2006), we do not interpret the regression output but plot the conditional effect of i-voting on turnout over time (see Figure 3). For Geneva (see the left panel), we see that the effect of i-voting on turnout, while marginally decreasing with time, is statistically insignificant across all calendar years. The picture is somewhat more complex in the case of Zurich (see the right panel). Surprisingly, the interaction suggests that i-voting negatively affected turnout in Zurich in the first two years. We should not read too much into this, however, given the very low number of i-voting trials on which this result is based (see the histogram in the background of the panel). In all other years, the effect is statistically insignificant. On balance, this suggests that there is no effect heterogeneity due to novelty or increased salience of security aspects, neither in Geneva nor in Zurich. In section D of the online supplement we provide additional evidence against effect heterogeneity. In particular, we show that the conclusion remains unchanged if we allow for non-linear changes of the treatment effect over time. We also

[12]In an ideal world, the possibility of heterogeneous effects due to variation in security concerns could be evaluated on the basis of survey-based estimates of the extent to which citizens trust i-voting. However, no such survey evidence exists on a time-series basis, let alone of a quality that would allow for representative estimates at municipality level.

Figure 3: Interaction with calendar year



(a) (b)

Note: The solid line indicates the effect of i-voting on turnout conditional on the calendar year and the dashed lines give the 95% confidence intervals. The underlying histogram gives the number of i-voting trials per calendar year. The coefficients on the product terms are -0.079 (clustered standard error=0.104) and 0.233 (0.25) for the Genevan and the Zurich model, respectively.

show evidence against effect heterogeneity due to the non-constant, on-and-off type of availability of i-voting in some of the municipalities.

7 Conclusion

This article presented evidence that the introduction of i-voting did not raise turnout in extended trials in two Swiss cantons, Geneva and Zurich, where i-voting was made available in addition to voting at the polling station and postal voting. To estimate the causal effect of i-voting on turnout we used a difference-in-differences approach that exploits federal legislation which in both Geneva and Zurich resulted in some municipalities taking part in the trials and others not. Since this design accounts for municipality- and referendum-specific confounders, as well as smoothly changing factors, we believe it provides more robust causal estimates compared to previous studies. Investigations into the plausibility of the causal identification assumption and a number of robustness checks bolster our confidence in the results.

Our findings suggest several new insights. First, the fact that i-voting did not raise turnout in Geneva and Zurich suggests that while i-voting may make voting slightly more convenient than postal voting, the convenience added by i-voting is too limited to

raise turnout.[13] Second, voters seem unmoved by the appeal of new voting technology, which some proponents hoped would entice new voters to the polls. While some voters may prefer to cast their vote using modern communications technology, according to our findings, they would most likely have voted anyway had online voting not been on offer, be it by post or at the polling station. There is no "pull" of the Internet, at least when it comes to voting.

Of course, as with all research, this study has its limitations. First, while by design we can exclude confounding due to variables that are fixed over time, time-specific, and smoothly changing, we cannot fully preclude the possibility of omitted variable bias due to time-varying variables. To account for this, we introduced controls for factors such as wealth and age structure in alternative models, and re-estimated the difference-in-differences based on matched samples. In each case, we found similar results. Nevertheless, as with all observational work, caution is justified.

Second, we focused on the effects of i-voting on aggregate turnout, and this may mask turnout effects among some special voter groups. In two models, we explicitly looked at turnout among young and elderly voters, finding that the null result holds also among these groups of voters. However, due to data limitations, we could not explicitly consider turnout effects on people with limited mobility or expatriates, two groups of voters for which i-voting is particularly likely to affect turnout.

Finally, we looked at electoral reform in a single country, Switzerland. As argued repeatedly, Switzerland is special in that voting is fairly convenient even without i-voting, given the availability of postal voting. It is important to stress that our study offers no grounds for concluding that i-voting does not affect turnout in a more standard scenario where the only alternative to i-voting is voting at the polling station. That said, there are some more grounds for optimism that our conclusion that i-voting does not increase turnout *in the presence of postal voting* holds more generally. It may not, of course, apply in all contexts. For example, Switzerland has a well-functioning and trusted postal

[13] It is also possible that distrust in online ballots prevented a turnout increase, though the fact that i-voting's effect remained stable over time despite increasing security concerns makes this unlikely.

system, and i-voting may make more of a positive contribution to voter turnout when postal voting is available but the postal system is inefficient and untrustworthy. But assuming a well-functioning postal system, we believe that our study offers relatively good grounds for extrapolation. First, many existing studies of the i-voting turnout effect have looked at pilot projects. However, the results of pilot projects may well not replicate in the longer run due to novelty effects. By contrast, the cases we analyzed rank among the few with a long-term experience with i-voting. Thus, the present study is less prone to the common problem of extrapolation from pilot projects.

Second, this study looked exclusively at the effects of i-voting in referendum votes. However, there are in our view few good reasons why i-voting's turnout effect would be different when it comes to elections. Finally, Switzerland admittedly constitutes a special case due to its low turnout. However, if anything, low turnout should make it more, not less, likely to find a turnout effect. If turnout is already high, the potential for convenience voting reforms to increase turnout is lower (Karp & Banducci 2000). From this perspective, i-voting should have been particularly likely to increase turnout in the Swiss cases we analyzed. The fact that it did not suggests that i-voting is even less likely to increase participation in settings with higher turnout rates.

Our study has several implications for policymaking. If we take our findings at face value, adding i-voting to postal voting appears to make little sense, at least from a turnout perspective. The earlier introduction of postal voting in Switzerland was shown to have increased turnout by about 4 percentage points (Luechinger, Rosinger & Stutzer 2007). Similarly sized effects have been found in other countries (Gronke et al. 2008). This study suggests that the further addition of i-voting is unlikely to further increase turnout. I-voting may have other advantages, especially in terms of electoral administration. But if policymakers' main aim is to increase turnout, they may be better advised to invest resources otherwise.

Further, our study offers some grounds for speculation that the old-fashioned postage stamp may even beat i-voting in terms of increasing turnout, as suggested by Norris (2005). Our results suggest that the difference in convenience between i-voting and postal

voting and the appeal of new technology matter little for voters' decision to turn out. At the same time, postal voting often raises fewer security concerns, and as we have argued, concerns about the security of online voting may offset potential gains due to added convenience or, in an extreme case, even decrease overall turnout. Further, postal voting extends to a broader audience because it does not presuppose Internet access and IT skills. This may well mean that postal voting outcompetes i-voting in terms of increasing turnout. However, our study did not directly contrast the effects of i-voting and postal voting, and so this remains speculation.

We conclude with a call for more research on i-voting's implications for turnout. A replication study testing the effect of adding i-voting to postal voting in a different context would be useful to (dis-)confirm the results of this study. Furthermore, future research should more fully explore the causal effects of i-voting on turnout in low convenience settings where the only alternative is voting at the polling station. Especially useful would be studies that directly compare the effects of i-voting and postal voting. Finally, another aspect that in our view merits increased academic attention is i-voting's effect on political participation among special voter groups, in particular among people with limited mobility, military personnel stationed abroad, and expatriates more generally.

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